

SCIENCE

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ADDRESS OF THE PRESIDENT OF THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE¹

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TWENTY-FIVE years ago a great change was made in the practise of the British Association. From the foundation of our society until 1884 its meetings had always been held in the British Isles; in that year, however, the association met in Montreal, and a step was taken which changed us from an insular into an imperial association. For this change, which now I think meets with nothing but approval, Canada is mainly responsible. Men of science welcome it for the increased opportunities it gives them of studying under the most pleasant and favorable conditions different parts of our empire, of making new friends; such meetings as these not only promote the progress of science, but also help to strengthen the bonds which bind together the different portions of the king's dominions.

This year, for the third time in a quarter of a century, we are meeting in Canada. As if to give us an object lesson in the growth of empire, you in Winnipeg took the opportunity at our first meeting in Canada in 1884 to invite our members to visit Manitoba and see for themselves the development of the province at that time. Those who were fortunate enough to be your guests then as well as now are confronted with a change which must seem to them unexampled and almost incredible. Great cities have sprung up, immense areas have been converted from prairies to prosperous farms, flourishing industries have been started, and the population has quad-

¹ Winnipeg, 1909.

rupled. As the president of a scientific association I hope I may be pardoned if I point out that even the enterprise and energy of your people and the richness of your country would have been powerless to effect this change without the resources placed at their disposal by the labors of men of science.

The eminence of my predecessors in the chair at the meetings of the British Association in Canada makes my task this evening a difficult one. The meeting at Montreal was presided over by Lord Rayleigh, who, like Lord Kelvin, his colleague in the chair of Section A at that meeting, has left the lion's mark on every department of physics, and who has shown that, vast as is the empire of physics, there are still men who can extend its frontiers in all of the many regions under its sway. It has been my lot to succeed Lord Rayleigh in other offices as well as this, and I know how difficult a man he is to follow.

The president of the second meeting in Canada—that held in 1897 at Toronto—was Sir John Evans, one of those men who, like Boyle, Cavendish, Darwin, Joule and Huggins, have, from their own resources and without the aid derived from official positions or from the universities, made memorable contributions to science: such men form one of the characteristic features of British science. May we not hope that, as the knowledge of science and the interest taken in it increase, more of the large number of men of independent means in our country may be found working for the advancement of science, and thereby rendering services to the community no less valuable than the political, philanthropic and social work at which many of them labor with so much zeal and success?

I can, however, claim to have some experience of, at any rate, one branch of Canadian science, for it has been my privi-

lege to receive at the Cavendish Laboratory many students from your universities. Some of these have been holders of what are known as the 1851 scholarships. These scholarships are provided from the surplus of the Great Exhibition of 1851, and are placed at the disposal of most of the younger universities in the British Empire, to enable students to devote themselves for two or three years to original research in various branches of science. I have had many opportunities of seeing the work of these scholars, and I should like to put on record my opinion that there is no educational endowment in the country which has done or is doing better work.

I have had, as I said, the privilege of having as pupils students from your universities as well as from those of New Zealand, Australia and the United States, and have thus had opportunities of comparing the effect on the best men of the educational system in force at your universities with that which prevails in the older English universities. Well, as the result, I have come to the conclusion that there is a good deal in the latter system which you have been wise not to imitate. The chief evil from which we at Cambridge suffer and which you have avoided is, I am convinced, the excessive competition for scholarships which confronts our students at almost every stage of their education. You may form some estimate of the prevalence of these scholarships if I tell you that the colleges in the University of Cambridge alone give more than £35,000 a year in scholarships to undergraduates, and I suppose the case is much the same at Oxford. The result of this is that preparation for these scholarships dominates the education of the great majority of the cleverer boys who come to these universities, and indeed in some quarters it seems to be held that the chief duty of a schoolmaster, and the

best test of his efficiency, is to make his boys get scholarships. The preparation for the scholarship too often means that about two years before the examination the boy begins to specialize, and from the age of sixteen does little else than the subject, be it mathematics, classics, or natural science, for which he wishes to get a scholarship; then, on entering the university, he spends three or four years studying the same subject before he takes his degree, when his real life work ought to begin. How has this training fitted him for this work? I will take the case in which the system might perhaps be expected to show to greatest advantage, when his work is to be original research in the subject he has been studying. He has certainly acquired a very minute acquaintance with his subject—indeed, the knowledge possessed by some of the students trained under this system is quite remarkable, much greater than that of any other students I have ever met. But though he has acquired knowledge, the effect of studying one subject, and one subject only, for so long a time is too often to dull his enthusiasm for it, and he begins research with much of his early interest and keenness evaporated. Now there is hardly any quality more essential to success in research than enthusiasm. Research is difficult, laborious, often disheartening. The carefully designed apparatus refuses to work, it develops defects which may take months of patient work to rectify, the results obtained may appear inconsistent with each other and with every known law of nature, sleepless nights and laborious days may seem only to make the confusion more confounded, and there is nothing for the student to do but to take for his motto "It's dogged as does it," and plod on, comforting himself with the assurance that when success does come, the difficulties he has overcome will increase

the pleasure—one of the most exquisite men can enjoy—of getting some conception which will make all that was tangled, confused and contradictory clear and consistent. Unless he has enthusiasm to carry him on when the prospect seems almost hopeless and the labor and strain incessant, the student may give up his task and take to easier, though less important, pursuits.

I am convinced that no greater evil can be done to a young man than to dull his enthusiasm. In a very considerable experience of students of physics beginning research, I have met with more—many more—failures from lack of enthusiasm and determination than from any lack of knowledge or of what is usually known as cleverness.

This continual harping from an early age on one subject, which is so efficient in quenching enthusiasm, is much encouraged by the practise of the colleges to give scholarships for proficiency in one subject alone. I went through a list of the scholarships awarded in the University of Cambridge last winter, and, though there were 202 of them, I could only find three cases in which it was specified that the award was made for proficiency in more than one subject.

The premature specialization fostered by the preparation for these scholarships injures the student by depriving him of adequate literary culture, while when it extends, as it often does, to specialization in one or two branches of science, it retards the progress of science by tending to isolate one science from another. The boundaries between the sciences are arbitrary, and tend to disappear as science progresses. The principles of one science often find most striking and suggestive illustrations in the phenomena of another. Thus, for example, the physicist finds in astronomy that effects he has observed in the laboratory are illustrated on the grand scale in

the sun and stars. No better illustration of this could be given than Professor Hale's recent discovery of the Zeeman effect in the light from sunspots; in chemistry, too, the physicist finds in the behavior of whole series of reactions illustrations of the great laws of thermodynamics, while if he turns to the biological sciences he is confronted by problems, mostly unsolved, of unsurpassed interest. Consider for a moment the problem presented by almost any plant—the characteristic and often exquisite detail of flower, leaf and habit—and remember that the mechanism which controls this almost infinite complexity was once contained in a seed perhaps hardly large enough to be visible. We have here one of the most entrancing problems in chemistry and physics it is possible to conceive.

Again the specialization prevalent in schools often prevents students of science from acquiring sufficient knowledge of mathematics; it is true that most of those who study physics do some mathematics, but I hold that, in general, they do not do enough, and that they are not as efficient physicists as they would be if they had a wider knowledge of that subject. There seems at present a tendency in some quarters to discourage the use of mathematics in physics; indeed, one might infer, from the statements of some writers in quasi-scientific journals, that ignorance of mathematics is almost a virtue. If this is so, then surely of all the virtues this is the easiest and most prevalent.

I do not for a moment urge that the physicist should confine himself to looking at his problems from the mathematical point of view; on the contrary, I think a famous French mathematician and physicist was guilty of only slight exaggeration when he said that no discovery was really important or properly understood by

its author unless and until he could explain it to the first man he met in the street.

But two points of view are better than one, and the physicist who is also a mathematician possesses a most powerful instrument for scientific research with which many of the greatest discoveries have been made; for example, electric waves were discovered by mathematics long before they were detected in the laboratory. He has also at his command a language clear, concise and universal, and there is no better way of detecting ambiguities and discrepancies in his ideas than by trying to express them in this language. Again, it often happens that we are not able to appreciate the full significance of some physical discovery until we have subjected it to mathematical treatment, when we find that the effect we have discovered involves other effects which have not been detected, and we are able by this means to duplicate the discovery. Thus James Thomson, starting from the fact that ice floats on water, showed that it follows by mathematics that ice can be melted and water prevented from freezing by pressure. This effect, which was at that time unknown, was afterwards verified by his brother, Lord Kelvin. Multitudes of similar duplication of physical discoveries by mathematics could be quoted.

I have been pleading in the interests of physics for a greater study of mathematics by physicists. I would also plead for a greater study of physics by mathematicians in the interest of pure mathematics.

The history of pure mathematics shows that many of the most important branches of the subject have arisen from the attempts made to get a mathematical solution of a problem suggested by physics. Thus the differential calculus arose from attempts to deal with the problem of moving bodies. Fourier's theorem resulted from

attempts to deal with the vibrations of strings and the conduction of heat; indeed, it would seem that the most fruitful crop of scientific ideas is produced by cross-fertilization between the mind and some definite fact, and that the mind by itself is comparatively unproductive.

I think, if we could trace the origin of some of our most comprehensive and important scientific ideas, it would be found that they arose in the attempt to find an explanation of some apparently trivial and very special phenomenon; when once started the ideas grew to such generality and importance that their modest origin could hardly be suspected. Water vapor we know will refuse to condense into rain unless there are particles of dust to form nuclei; so an idea before taking shape seems to require a nucleus of solid fact round which it can condense.

I have ventured to urge the closer union between mathematics and physics, because I think of late years there has been some tendency for these sciences to drift apart, and that the workers in applied mathematics are relatively fewer than they were some years ago. This is no doubt due to some extent to the remarkable developments made in the last few years in experimental physics on the one hand and in the most abstract and metaphysical parts of pure mathematics on the other. The fascination of these has drawn workers to the frontiers of these regions who would otherwise have worked nearer the junction of the two. In part, too, it may be due to the fact that the problems with which the applied mathematician has to deal are exceedingly difficult, and many may have felt that the problems presented by the older physics have been worked over so often by men of the highest genius that there was but little chance of any problem which they could have any hope of solving being left.

But the newer developments of physics have opened virgin ground which has not yet been worked over and which offers problems to the mathematician of great interest and novelty—problems which will suggest and require new methods of attack, the development of which will advance pure mathematics as well as physics.

I have alluded to the fact that pure mathematicians have been indebted to the study of concrete problems for the origination of some of their most valuable conceptions; but though no doubt pure mathematicians are in many ways very exceptional folk, yet in this respect they are very human. Most of us need to tackle some definite difficulty before our minds develop whatever powers they may possess. This is true for even the youngest of us, for our school boys and school girls, and I think the moral to be drawn from it is that we should aim at making the education in our schools as little bookish and as practical and concrete as possible.

I once had an illustration of the power of the concrete in stimulating the mind which made a very lasting impression upon me. One of my first pupils came to me with the assurance from his previous teacher that he knew little and cared less about mathematics, and that he had no chance of obtaining a degree in that subject. For some time I thought this estimate was correct, but he happened to be enthusiastic about billiards, and when we were reading that part of mechanics which deals with the collision of elastic bodies I pointed out that many of the effects he was constantly observing were illustrations of the subject we were studying. From that time he was a changed man. He had never before regarded mathematics as anything but a means of annoying innocent undergraduates; now, when he saw what important results it could obtain, he became en-

thusiastic about it, developed very considerable mathematical ability, and, though he had already wasted two out of his three years at college, took a good place in the mathematical tripos.

It is possible to read books, to pass examinations without the higher qualities of the mind being called into play. Indeed, I doubt if there is any process in which the mind is more quiescent than in reading without interest. I might appeal to the widespread habit of reading in bed as a prevention of insomnia as a proof of this. But it is not possible for a boy to make a boat or for a girl to cook a dinner without using their brains. With practical things the difficulties have to be surmounted, the boat must be made watertight, the dinner must be cooked, while in reading there is always the hope that the difficulties which have been slurred over will not be set in the examination.

I think it was Helmholtz who said that often in the course of a research more thought and energy were spent in reducing a refractory piece of brass to order than in devising the method or planning the scheme of campaign. This constant need for thought and action gives to original research in any branch of experimental science great educational value even for those who will not become professional men of science. I have had considerable experience with students beginning research in experimental physics, and I have always been struck by the quite remarkable improvement in judgment, independence of thought and maturity produced by a year's research. Research develops qualities which are apt to atrophy when the student is preparing for examinations, and, quite apart from the addition of new knowledge to our store, is of the greatest importance as a means of education.

IT is the practise in many universities to

make special provision for the reception of students from other universities who wish to do original research or to study the more advanced parts of their subject, and considerable numbers of such students migrate from one university to another. I think it would be a good thing if this practise were to extend to students at an earlier stage in their career; especially should I like to see a considerable interchange of students between the universities in the mother country and those in the colonies.

I am quite sure that many of our English students, especially those destined for public life, could have no more valuable experience than to spend a year in one or other of your universities, and I hope some of your students might profit by a visit to ours.

I can think of nothing more likely to lead to a better understanding of the feelings, the sympathies, and, what is not less important, the prejudices, of one country by another, than by the youths of those countries spending a part of their student life together. Undergraduates as a rule do not wear a mask either of politeness or any other material, and have probably a better knowledge of each other's opinions and points of view—in fact, know each other better than do people of riper age. To bring this communion of students about there must be cooperation between the universities throughout the empire; there must be recognition of each other's examinations, residence and degrees. Before this can be accomplished there must, as my friend Mr. E. B. Sargent pointed out in a lecture given at the McGill University, be cooperation and recognition between the universities in each part of the empire. I do not mean for a moment that all universities in a country should be under one government. I am a strong believer in the individuality of universities, but I do not

think this is in any way inconsistent with the policy of an open door from one university to every other in the empire.

It has usually been the practise of the president of this association to give some account of the progress made in the last few years in the branch of science which he has the honor to represent.

I propose this evening to follow that precedent and to attempt to give a very short account of some of the more recent developments of physics, and the new conceptions of physical processes to which they have led.

The period which has elapsed since the association last met in Canada has been one of almost unparalleled activity in many branches of physics, and many new and unsuspected properties of matter and electricity have been discovered. The history of this period affords a remarkable illustration of the effect which may be produced by a single discovery; for it is, I think, to the discovery of the Röntgen rays that we owe the rapidity of the progress which has recently been made in physics. A striking discovery like that of the Röntgen rays acts much like the discovery of gold in a sparsely populated country; it attracts workers who come in the first place for the gold, but who may find that the country has other products, other charms, perhaps even more valuable than the gold itself. The country in which the gold was discovered in the case of the Röntgen rays was the department of physics dealing with the discharge of electricity through gases, a subject which, almost from the beginning of electrical science, had attracted a few enthusiastic workers, who felt convinced that the key to unlock the secret of electricity was to be found in a vacuum tube. Röntgen, in 1895, showed that when electricity passed through such a tube, the tube emitted rays which could pass through

bodies opaque to ordinary light; which could, for example, pass through the flesh of the body and throw a shadow of the bones on a suitable screen. The fascination of this discovery attracted many workers to the subject of the discharge of electricity through gases, and led to great improvements in the instruments used in this type of research. It is not, however, to the power of probing dark places, important though this is, that the influence of Röntgen rays on the progress of science has mainly been due; it is rather because these rays make gases, and, indeed, solids and liquids, through which they pass conductors of electricity. It is true that before the discovery of these rays other methods of making gases conductors were known, but none of these was so convenient for the purposes of accurate measurement.

The study of gases exposed to Röntgen rays has revealed in such gases the presence of particles charged with electricity; some of these particles are charged with positive, others with negative electricity.

The properties of these particles have been investigated; we know the charge they carry, the speed with which they move under an electric force, the rate at which the oppositely charged ones recombine, and these investigations have thrown a new light, not only on electricity, but also on the structure of matter.

We know from these investigations that electricity, like matter, is molecular in structure, that just as a quantity of hydrogen is a collection of an immense number of small particles called molecules, so a charge of electricity is made up of a great number of small charges, each of a perfectly definite and known amount.

Helmholtz said in 1880 that in his opinion the evidence in favor of the molecular constitution of electricity was even stronger than that in favor of the molecular consti-

tution of matter. How much stronger is that evidence now, when we have measured the charge on the unit and found it to be the same from whatever source the electricity is obtained. Nay, further, the molecular theory of matter is indebted to the molecular theory of electricity for the most accurate determination of its fundamental quantity, the number of molecules in any given quantity of an elementary substance.

The great advantage of the electrical methods for the study of the properties of matter is due to the fact that whenever a particle is electrified it is very easily identified, whereas an uncharged molecule is most elusive; and it is only when these are present in immense numbers that we are able to detect them. A very simple calculation will illustrate the difference in our power of detecting electrified and un-electrified molecules. The smallest quantity of unelectrified matter ever detected is probably that of neon, one of the inert gases of the atmosphere. Professor Strutt has shown that the amount of neon in one twentieth of a cubic centimeter of the air at ordinary pressures can be detected by the spectroscope; Sir William Ramsay estimates that the neon in the air only amounts to one part of neon in 100,000 parts of air, so that the neon in one twentieth of a cubic centimeter of air would only occupy at atmospheric pressure a volume of half a millionth of a cubic centimeter. When stated in this form the quantity seems exceedingly small, but in this small volume there are about ten million million molecules. Now the population of the earth is estimated at about fifteen hundred millions, so that the smallest number of molecules of neon we can identify is about 7,000 times the population of the earth. In other words, if we had no better test for the existence of a man than we have for that of an unelectrified molecule we should come

to the conclusion that the earth is uninhabited. Contrast this with our power of detecting electrified molecules. We can by the electrical method, even better by the cloud method of C. T. R. Wilson, detect the presence of three or four charged particles in a cubic centimeter. Rutherford has shown that we can detect the presence of a single α particle. Now the particle is a charged atom of helium; if this atom had been uncharged we should have required more than a million million of them, instead of one, before we should have been able to detect them.

We may, I think, conclude, since electrified particles can be studied with so much greater ease than unelectrified ones, that we shall obtain a knowledge of the ultimate structure of electricity before we arrive at a corresponding degree of certainty with regard to the structure of matter.

We have already made considerable progress in the task of discovering what the structure of electricity is. We have known for some time that of one kind of electricity—the negative—and a very interesting one it is. We know that negative electricity is made up of units all of which are of the same kind; that these units are exceedingly small compared with even the smallest atom, for the mass of the unit is only $\frac{1}{1700}$ part of the mass of an atom of hydrogen; that its radius is only 10^{-13} centimeter, and that these units, "corpuscles" as they have been called, can be obtained from all substances. The size of these corpuscles is on an altogether different scale from that of atoms; the volume of a corpuscle bears to that of the atom about the same relation as that of a speck of dust to the volume of this room. Under suitable conditions they move at enormous speeds which approach in some instances the velocity of light.

The discovery of these corpuscles is an

interesting example of the way nature responds to the demands made upon her by mathematicians. Some years before the discovery of corpuscles it had been shown by a mathematical investigation that the mass of a body must be increased by a charge of electricity. This increase, however, is greater for small bodies than for large ones, and even bodies as small as atoms are hopelessly too large to show any appreciable effect; thus the result seemed entirely academic. After a time corpuscles were discovered, and these are so much smaller than the atom that the increase in mass due to the charge becomes not merely appreciable, but so great that, as the experiments of Kaufmann and Bucherer have shown, the whole of the mass of the corpuscle arises from its charge.

We know a great deal about negative electricity; what do we know about positive electricity? Is positive electricity molecular in structure? Is it made up into units, each unit carrying a charge equal in magnitude though opposite in sign to that carried by a corpuscle? Does, or does not, this unit differ, in size and physical properties, very widely from the corpuscle? We know that by suitable processes we can get corpuscles out of any kind of matter, and that the corpuscles will be the same from whatever source they may be derived. Is a similar thing true for positive electricity? Can we get, for example, a positive unit from oxygen of the same kind as that we get from hydrogen?

For my own part, I think the evidence is in favor of the view that we can, although the nature of the unit of positive electricity makes the proof much more difficult than for the negative unit.

In the first place we find that the positive particles—"canalstrahlen" is their technical name—discovered by our distinguished guest, Dr. Goldstein, which are found when an electric discharge passes

through a highly rarefied gas, are, when the pressure is very low, the same, whatever may have been the gas in the vessel to begin with. If we pump out the gas until the pressure is too low to allow the discharge to pass, and then introduce a small quantity of gas and restart the discharge, the positive particles are the same whatever kind of gas may have been introduced.

I have, for example, put into the exhausted vessel oxygen, argon, helium, the vapor of carbon tetrachloride, none of which contain hydrogen, and found the positive particles to be the same as when hydrogen was introduced.

Some experiments made lately by Wellisch, in the Cavendish Laboratory, strongly support the view that there is a definite unit of positive electricity independent of the gas from which it is derived; these experiments were on the velocity with which positive particles move through mixed gases. If we have a mixture of methyl-iodide and hydrogen exposed to Röntgen rays, the effect of the rays on the methyl-iodide is so much greater than on the hydrogen that, even when the mixture contains only a small percentage of methyl-iodide, practically all the electricity comes from this gas, and not from the hydrogen.

Now if the positive particles were merely the residue left when a corpuscle had been abstracted from the methyl-iodide, these particles would have the dimensions of a molecule of methyl-iodide; this is very large and heavy, and would therefore move more slowly through the hydrogen molecules than the positive particles derived from hydrogen itself, which would, on this view, be of the size and weight of the light hydrogen molecules. Wellisch found that the velocities of both the positive and negative particles through the mixture were the same as the velocities through pure hydrogen, although in the one case the ions had originated from methyl-iodide and in the

other from hydrogen; a similar result was obtained when carbon tetrachloride, or mercury methyl, was used instead of methyl-iodide. These and similar results lead to the conclusion that the atoms of the different chemical elements contain definite units of positive as well as of negative electricity, and that the positive electricity, like the negative, is molecular in structure.

The investigations made on the unit of positive electricity show that it is of quite a different kind from the unit of negative, the mass of the negative unit is exceedingly small compared with any atom, the only positive units that up to the present have been detected are quite comparable in mass with the mass of an atom of hydrogen; in fact they seem equal to it. This makes it more difficult to be certain that the unit of positive electricity has been isolated, for we have to be on our guard against its being a much smaller body attached to the hydrogen atoms which happen to be present in the vessel. If the positive units have a much greater mass than the negative ones, they ought not to be so easily deflected by magnetic forces when moving at equal speeds; and in general the insensibility of the positive particles to the influence of a magnet is very marked; though there are cases when the positive particles are much more readily deflected, and these have been interpreted as proving the existence of positive units comparable in mass with the negative ones. I have found, however, that in these cases the positive particles are moving very slowly, and that the ease with which they are deflected is due to the smallness of the velocity and not to that of the mass. It should, however, be noted that M. Jean Beequerel has observed in the absorption spectra of some minerals, and Professor Wood in the rotation of the plane of polarization by sodium vapor, effects which could be explained by the presence in the substances of positive units comparable in

mass with corpuscles. This, however, is not the only explanation which can be given of these effects, and at present the smallest positive electrified particles of which we have direct experimental evidence have masses comparable with that of an atom of hydrogen.

A knowledge of the mass and size of the two units of electricity, the positive and the negative, would give us the material for constructing what may be called a molecular theory of electricity, and would be a starting-point for a theory of the structure of matter; for the most natural view to take, as a provisional hypothesis, is that matter is just a collection of positive and negative units of electricity, and that the forces which hold atoms and molecules together, the properties which differentiate one kind of matter from another, all have their origin in the electrical forces exerted by positive and negative units of electricity, grouped together in different ways in the atoms of the different elements.

As it would seem that the units of positive and negative electricity are of very different sizes, we must regard matter as a mixture containing systems of very different types, one type corresponding to the small corpuscle, the other to the large positive unit.

Since the energy associated with a given charge is greater the smaller the body on which the charge is concentrated, the energy stored up in the negative corpuscles will be far greater than that stored up by the positive. The amount of energy which is stored up in ordinary matter in the form of the electrostatic potential energy of its corpuscles is, I think, not generally realized. All substances give out corpuscles, so that we may assume that each atom of a substance contains at least one corpuscle. From the size and the charge on the corpuscle, both of which are known, we find that each cor-

puscle has 8×10^{-7} ergs of energy; this is on the supposition that the usual expressions for the energy of a charged body hold when, as in the case of a corpuscle, the charge is reduced to one unit. Now in one gram of hydrogen there are about 6×10^{23} atoms, so if there is only one corpuscle in each atom the energy due to the corpuscles in a gram of hydrogen would be 48×10^{16} ergs, or 11×10^9 calories. This is more than seven times the heat developed by one gram of radium, or than that developed by the burning of five tons of coal. Thus we see that even ordinary matter contains enormous stores of energy; this energy is fortunately kept fast bound by the corpuscles; if at any time an appreciable fraction were to get free the earth would explode and become a gaseous nebula.

The matter of which I have been speaking so far is the material which builds up the earth, the sun, and the stars, the matter studied by the chemist, and which he can represent by a formula; this matter occupies, however, but an insignificant fraction of the universe, it forms but minute islands in the great ocean of the ether, the substance with which the whole universe is filled.

The ether is not a fantastic creation of the speculative philosopher; it is as essential to us as the air we breathe. For we must remember that we on this earth are not living on our own resources; we are dependent from minute to minute upon what we are getting from the sun, and the gifts of the sun are conveyed to us by the ether. It is to the sun that we owe not merely night and day, springtime and harvest, but it is the energy of the sun, stored up in coal, in waterfalls, in food, that practically does all the work of the world.

How great is the supply the sun lavishes upon us becomes clear when we consider that the heat received by the earth under a high sun and a clear sky is equivalent, ac-

cording to the measurements of Langley, to about 7,000 horse-power per acre. Though our engineers have not yet discovered how to utilize this enormous supply of power, they will, I have not the slightest doubt, ultimately succeed in doing so; and when coal is exhausted and our water-power inadequate, it may be that this is the source from which we shall derive the energy necessary for the world's work. When that comes about, our centers of industrial activity may perhaps be transferred to the burning deserts of the Sahara, and the value of land determined by its suitability for the reception of traps to catch sun-beams.

This energy, in the interval between its departure from the sun and its arrival at the earth, must be in the space between them. Thus this space must contain something which, like ordinary matter, can store up energy, which can carry at an enormous pace the energy associated with light and heat, and which can, in addition, exert the enormous stresses necessary to keep the earth circling round the sun and the moon round the earth.

The study of this all-pervading substance is perhaps the most fascinating and important duty of the physicist.

On the electromagnetic theory of light, now universally accepted, the energy streaming to the earth travels through the ether in electric waves; thus practically the whole of the energy at our disposal has at one time or another been electrical energy. The ether must, then, be the seat of electrical and magnetic forces. We know, thanks to the genius of Clerk Maxwell, the founder and inspirer of modern electrical theory, the equations which express the relation between these forces, and although for some purposes these are all we require, yet they do not tell us very much about the nature of the ether.

The interest inspired by equations, too, in some minds is apt to be somewhat Platonic; and something more grossly mechanical—a model, for example, is felt by many to be more suggestive and manageable, and for them a more powerful instrument of research, than a purely analytical theory.

Is the ether dense or rare? Has it a structure? Is it at rest or in motion? are some of the questions which force themselves upon us.

Let us consider some of the facts known about the ether. When light falls on a body and is absorbed by it, the body is pushed forward in the direction in which the light is traveling, and if the body is free to move it is set in motion by the light. Now it is a fundamental principle of dynamics that when a body is set moving in a certain direction, or, to use the language of dynamics, acquires momentum in that direction, some other mass must lose the same amount of momentum; in other words, the amount of momentum in the universe is constant. Thus when the body is pushed forward by the light some other system must have lost the momentum the body acquires, and the only other system available is the wave of light falling on the body; hence we conclude that there must have been momentum in the wave in the direction in which it is traveling. Momentum, however, implies mass in motion. We conclude, then, that in the ether through which the wave is moving there is mass moving with the velocity of light. The experiments made on the pressure due to light enable us to calculate this mass, and we find that in a cubic kilometer of ether carrying light as intense as sunlight is at the surface of the earth, the mass moving is only about one fifty-millionth of a milligram. We must be careful not to confuse this with the mass of a cubic kilometer of ether; it is only the mass moved when the light passes through it; the vast majority of the ether is

left undisturbed by the light. Now, on the electro-magnetic theory of light, a wave of light may be regarded as made up of groups of lines of electric force moving with the velocity of light; and if we take this point of view we can prove that the mass of ether per cubic centimeter carried along is proportional to the energy possessed by these lines of electric force per cubic centimeter, divided by the square of the velocity of light. But though lines of electric force carry some of the ether along with them as they move, the amount so carried, even in the strongest electric fields we can produce, is but a minute fraction of the ether in their neighborhood.

This is proved by an experiment made by Sir Oliver Lodge in which light was made to travel through an electric field in rapid motion. If the electric field had carried the whole of the ether with it, the velocity of the light would have been increased by the velocity of the electric field. As a matter of fact no increase whatever could be detected, though it would have been registered if it had amounted to one-thousandth part of that of the field.

The ether carried along by a wave of light must be an exceedingly small part of the volume through which the wave is spread. Parts of this volume are in motion, but by far the greater part is at rest; thus in the wave front there can not be uniformity, at some parts the ether is moving, at others it is at rest—in other words, the wave front must be more analogous to bright specks on a dark ground than to a uniformly illuminated surface.

The place where the density of the ether carried along by an electric field rises to its highest value is close to a corpuscle, for round the corpuscles are by far the strongest electric fields of which we have any knowledge. We know the mass of the corpuscle, we know from Kaufmann's experi-

ments that this arises entirely from the electric charge, and is therefore due to the ether carried along with the corpuscle by the lines of force attached to it.

A simple calculation shows that one half of this mass is contained in a volume seven times that of a corpuscle. Since we know the volume of the corpuscle as well as the mass, we can calculate the density of the ether attached to the corpuscle; doing so, we find it amounts to the prodigious value of about 5×10^{10} , or about 2,000 million times that of lead. Sir Oliver Lodge, by somewhat different considerations, has arrived at a value of the same order of magnitude.

Thus around the corpuscle ether must have an extravagant density: whether the density is as great as this in other places depends upon whether the ether is compressible or not. If it is compressible, then it may be condensed round the corpuscles, and there have an abnormally great density; if it is not compressible, then the density in free space can not be less than the number I have just mentioned.

With respect to this point we must remember that the forces acting on the ether close to the corpuscle are prodigious. If the ether were, for example, an ideal gas whose density increased in proportion to the pressure, however great the pressure might be, then if, when exposed to the pressures which exist in some directions close to the corpuscle, it had the density stated above, its density under atmospheric pressure would only be about 8×10^{-16} , or a cubic kilometer would have a mass less than a gram; so that instead of being almost incomparably denser than lead, it would be almost incomparably rarer than the lightest gas.

I do not know at present of any effect which would enable us to determine whether ether is compressible or not. And

although at first sight the idea that we are immersed in a medium almost infinitely denser than lead might seem inconceivable, it is not so if we remember that in all probability matter is composed mainly of holes. We may, in fact, regard matter as possessing a bird-cage kind of structure in which the volume of the ether disturbed by the wires when the structure is moved is infinitesimal in comparison with the volume enclosed by them. If we do this, no difficulty arises from the great density of the ether; all we have to do is to increase the distance between the wires in proportion as we increase the density of the ether.

Let us now consider how much ether is carried along by ordinary matter, and what effects this might be expected to produce.

The simplest electrical system we know, an electrified sphere, has attached to it a mass of ether proportional to its potential energy, and such that if the mass were to move with the velocity of light its kinetic energy would equal the electrostatic potential energy of the particle. This result can be extended to any electrified system, and it can be shown that such a system binds a mass of the ether proportional to its potential energy. Thus a part of the mass of any system is proportional to the potential energy of the system.

The question now arises, Does this part of the mass add anything to the weight of the body? If the ether were not subject to gravitational attraction it certainly would not; and even if the ether were ponderable, we might expect that as the mass is swimming in a sea of ether it would not increase the weight of the body to which it is attached. But if it does not, then a body with a large amount of potential energy may have an appreciable amount of its mass in a form which does not increase its weight, and thus the weight of a given mass of it may be less than that of an equal mass of

some substance with a smaller amount of potential energy. Thus the weights of equal masses of these substances would be different. Now, experiments with pendulums, as Newton pointed out, enable us to determine with great accuracy the weights of equal masses of different substances. Newton himself made experiments of this kind, and found that the weights of equal masses were the same for all the materials he tried. Bessel, in 1830, made some experiments on this subject which are still the most accurate we possess, and he showed that the weights of equal masses of lead, silver, iron, brass did not differ by as much as one part in 60,000.

The substances tried by Newton and Bessel did not, however, include any of those substances which possess the marvellous power of radioactivity; the discovery of these came much later, and is one of the most striking achievements of modern physics.

These radioactive substances are constantly giving out large quantities of heat, presumably at the expense of their potential energy; thus when these substances reach their final non-radioactive state their potential energy must be less than when they were radioactive. Professor Rutherford's measurements show that the energy emitted by one gram of radium in the course of its degradation to non-radioactive forms is equal to the kinetic energy of a mass of one thirteenth of a milligram moving with the velocity of light.

This energy, according to the rule I have stated, corresponds to a mass of one thirteenth of a milligram of the ether, and thus a gram of radium in its radioactive state must have at least one thirteenth of a milligram more of ether attached to it than when it has been degraded into the non-radioactive forms. Thus if this ether does not increase the weight of the radium, the

ratio of mass to weight for radium would be greater by about one part in 13,000 than for its non-radioactive products.

I attempted several years ago to find the ratio of mass to weight for radium by swinging a little pendulum, the bob of which was made of radium. I had only a small quantity of radium, and was not, therefore, able to attain any great accuracy. I found that the difference, if any, in the ratio of the mass to weight between radium and other substances was not more than one part in 2,000. Lately we have been using at the Cavendish Laboratory a pendulum whose bob was filled with uranium oxide. We have got good reasons for supposing that uranium is a parent of radium, so that the great potential energy and large ethereal mass possessed by the radium will be also in the uranium; the experiments are not yet completed. It is, perhaps, expecting almost too much to hope that the radioactive substances may add to the great services they have already done to science by furnishing the first case in which there is some differentiation in the action of gravity.

The mass of ether bound by any system is such that if it were to move with the velocity of light its kinetic energy would be equal to the potential energy of the system. This result suggests a new view of the nature of potential energy. Potential energy is usually regarded as essentially different from kinetic energy. Potential energy depends on the configuration of the system, and can be calculated from it when we have the requisite data; kinetic energy, on the other hand, depends upon the velocity of the system. According to the principle of the conservation of energy the one form can be converted into the other at a fixed rate of exchange, so that when one unit of one kind disappears a unit of the other simultaneously appears.

Now in many cases this rule is all that we require to calculate the behavior of the system, and the conception of potential energy is of the utmost value in making the knowledge derived from experiment and observation available for mathematical calculation. It must, however, I think, be admitted that from the purely philosophical point of view it is open to serious objection. It violates, for example, the principle of continuity. When a thing changes from a state *A* to a different state *B*, the principle of continuity requires that it must pass through a number of states intermediate between *A* and *B*, so that the transition is made gradually, and not abruptly. Now, when kinetic energy changes into potential, although there is no discontinuity in the quantity of the energy, there is in its quality, for we do not recognize any kind of energy intermediate between that due to the motion and that due to the position of the system, and some portions of energy are supposed to change *per saltum* from the kinetic to the potential form. In the case of the transition of kinetic energy into heat energy in a gas, the discontinuity has disappeared with a fuller knowledge of what the heat energy in a gas is due to. When we were ignorant of the nature of this energy, the transition from kinetic into thermal energy seemed discontinuous; but now we know that this energy is the kinetic energy of the molecules of which the gas is composed, so that there is no change in the type of energy when the kinetic energy of visible motion is transformed into the thermal energy of a gas—it is just the transference of kinetic energy from one body to another.

If we regard potential energy as the kinetic energy of portions of the ether attached to the system, then all energy is kinetic energy, due to the motion of matter or of portions of ether attached to the mat-

ter. I showed, many years ago, in my "Applications of Dynamics to Physics and Chemistry," that we could imitate the effects of the potential energy of a system by means of the kinetic energy of invisible systems connected in an appropriate manner with the main system, and that the potential energy of the visible universe may in reality be the kinetic energy of an invisible one connected up with it. We naturally suppose that this invisible universe is the luminiferous ether, that portions of the ether in rapid motion are connected with the visible systems, and that their kinetic energy is the potential energy of the systems.

We may thus regard the ether as a bank in which we may deposit energy and withdraw it at our convenience. The mass of the ether attached to the system will change as the potential energy changes, and thus the mass of a system whose potential energy is changing can not be constant; the fluctuations in mass under ordinary conditions are, however, so small that they can not be detected by any means at present at our disposal. Inasmuch as the various forms of potential energy are continually being changed into heat energy, which is the kinetic energy of the molecules of matter, there is a constant tendency for the mass of a system such as the earth or the sun to diminish, and thus as time goes on for the mass of ether gripped by the material universe to become smaller and smaller; the rate at which it would diminish would, however, get slower as time went on, and there is no reason to think that it would ever get below a very large value.

Radiation of light and heat from an incandescent body like the sun involves a constant loss of mass by the body. Each unit of energy radiated carries off its quota of mass, but as the mass ejected from the sun

per year is only one part in 20 billionths (1 in 2×10^{13}) of the mass of the sun, and as this diminution in mass is not necessarily accompanied by any decrease in its gravitational attraction, we can not expect to be able to get any evidence of this effect.

As our knowledge of the properties of light has progressed, we have been driven to recognize that the ether, when transmitting light, possesses properties which, before the introduction of the electro-magnetic theory, would have been thought to be peculiar to an emission theory of light and to be fatal to the theory that light consists of undulations.

Take, for example, the pressure exerted by light. This would follow as a matter of course if we supposed light to be small particles moving with great velocities, for these, if they struck against a body, would manifestly tend to push it forward, while on the undulatory theory there seemed no reason why any effect of this kind should take place.

Indeed, in 1792, this very point was regarded as a test between the theories, and Bennet made experiments to see whether or not he could find any traces of this pressure. We now know that the pressure is there, and if Bennet's instrument had been more sensitive he must have observed it. It is perhaps fortunate that Bennet had not at his command more delicate apparatus. Had he discovered the pressure of light, it would have shaken confidence in the undulatory theory and checked that magnificent work at the beginning of the last century which so greatly increased our knowledge of optics.

As another example, take the question of the distribution of energy in a wave of light. On the emission theory the energy in the light is the kinetic energy of the light particles. Thus the energy of light is

made up of distinct units, the unit being the energy of one of the particles.

The idea that the energy has a structure of this kind has lately received a good deal of support. Planck, in a very remarkable series of investigations on the thermodynamics of radiation, pointed out that the expressions for the energy and entropy of radiant energy were of such a form as to suggest that the energy of radiation, like that of a gas on the molecular theory, was made up of distinct units, the magnitude of the unit depending on the color of the light; and on this assumption he was able to calculate the value of the unit, and from this deduce incidentally the value of Avogadro's constant—the number of molecules in a cubic centimeter of gas at standard temperature and pressure.

This result is most interesting and important because if it were a legitimate deduction from the second law of thermodynamics, it would appear that only a particular type of mechanism for the vibrators which give out light and the absorbers which absorb it could be in accordance with that law.

If this were so, then, regarding the universe as a collection of machines all obeying the laws of dynamics, the second law of thermodynamics would only be true for a particular kind of machine.

There seems, however, grave objection to this view, which I may illustrate by the case of the first law of thermodynamics, the principle of the conservation of energy. This must be true whatever be the nature of the machines which make up the universe, provided they obey the laws of dynamics, any application of the principle of the conservation of energy could not discriminate between one type of machine and another.

Now, the second law of thermodynamics, though not a dynamical principle in as

strict a sense as the law of the conservation of energy, is one that we should expect to hold for a collection of a large number of machines of any type, provided that we could not directly affect the individual machines, but could only observe the average effects produced by an enormous number of them. On this view, the second law, as well as the first, should be incapable of saying that the machines were of any particular type: so that investigations founded on thermodynamics, though the expressions they lead to may suggest—can not, I think, be regarded as proving—the unit structure of light energy.

It would seem as if in the application of thermodynamics to radiation some additional assumption has been implicitly introduced, for these applications lead to definite relations between the energy of the light of any particular wave-length and the temperature of the luminous body.

Now a possible way of accounting for the light emitted by hot bodies is to suppose that it arises from the collisions of corpuscles with the molecules of the hot body, but it is only for one particular law of force between the corpuscles and the molecules that the distribution of energy would be the same as that deduced by the second law of thermodynamics, so that in this case, as in the other, the results obtained by the application of thermodynamics to radiation would require us to suppose that the second law of thermodynamics is only true for radiation when the radiation is produced by mechanism of a special type.

Quite apart, however, from considerations of thermodynamics, we should expect that the light from a luminous source should in many cases consist of parcels, possessing, at any rate to begin with, a definite amount of energy. Consider, for example, the case of a gas like sodium vapor, emitting light of a definite wave-

length; we may imagine that this light, consisting of electrical waves, is emitted by systems resembling Leyden jars. The energy originally possessed by such a system will be the electrostatic energy of the charged jar. When the vibrations are started, this energy will be radiated away into space, the radiation forming a complex system, containing, if the jar has no electrical resistance, the energy stored up in the jar.

The amount of this energy will depend on the size of the jar and the quantity of electricity with which it is charged. With regard to the charge, we must remember that we are dealing with systems formed out of single molecules, so that the charge will only consist of one or two natural units of electricity, or, at all events, some small multiple of that unit, while for geometrically similar Leyden jars the energy for a given charge will be proportional to the frequency of the vibration; thus, the energy in the bundle of radiation will be proportional to the frequency of the vibration.

We may picture to ourselves the radiation as consisting of the lines of electric force which, before the vibrations were started, were held bound by the charges on the jar, and which, when the vibrations begin, are thrown into rhythmic undulations, liberated from the jar and travel through space with the velocity of light.

Now let us suppose that this system strikes against an uncharged condenser and gives it a charge of electricity, the charge on the plates of the condenser must be at least one unit of electricity, because fractions of this charge do not exist, and each unit charge will anchor a unit tube of force, which must come from the parcel of radiation falling upon it. Thus a tube in the incident light will be anchored by the condenser, and the parcel formed by

this tube will be anchored and withdrawn as a whole from the pencil of light incident on the condenser. If the energy required to charge up the condenser with a unit of electricity is greater than the energy in the incident parcel, the tube will not be anchored and the light will pass over the condenser and escape from it. These principles that radiation is made up of units, and that it requires a unit possessing a definite amount of energy to excite radiation in a body on which it falls, perhaps receive their best illustration in the remarkable laws governing secondary Röntgen radiation, recently discovered by Professor Barkla. Professor Barkla has found that each of the different chemical elements, when exposed to Röntgen rays, emit a definite type of secondary radiation whatever may have been the type of primary, thus lead emits one type, copper another, and so on; but these radiations are not excited at all if the primary radiation is of a softer type than the specific radiation emitted by the substance; thus the secondary radiation from lead being harder than that from copper; if copper is exposed to the secondary radiation from lead the copper will radiate, but lead will not radiate when exposed to copper. Thus, if we suppose that the energy in a unit of hard Röntgen rays is greater than that in one of soft, Barkla's results are strikingly analogous to those which would follow on the unit theory of light.

Though we have, I think, strong reasons for thinking that the energy in the light waves of definite wave-length is done up into bundles, and that these bundles, when emitted, all possess the same amount of energy, I do not think there is any reason for supposing that in any casual specimen of light of this wave-length, which may subsequent to its emission have been many times refracted or reflected, the bundles pos-

sess any definite amount of energy. For consider what must happen when a bundle is incident on a surface such as glass, when part of it is reflected and part transmitted. The bundle is divided into two portions, in each of which the energy is less than the incident bundle, and since these portions diverge and may ultimately be many thousands of miles apart, it would seem meaningless to still regard them as forming one unit. Thus the energy in the bundles of light, after they have suffered partial reflection, will not be the same as in the bundles when they were emitted. The study of the dimensions of these bundles, for example, the angle they subtend at the luminous source, is an interesting subject for investigation; experiments on interference between rays of light emerging in different directions from the luminous source would probably throw light on this point.

I now pass to a very brief consideration of one of the most important and interesting advances ever made in physics, and in which Canada, as the place of the labors of Professors Rutherford and Soddy, has taken a conspicuous part. I mean the discovery and investigation of radioactivity. Radioactivity was brought to light by the Röntgen rays. One of the many remarkable properties of these rays is to excite phosphorescence in certain substances, including the salts of uranium, when they fall upon them. Since Röntgen rays produce phosphorescence, it occurred to Becquerel to try whether phosphorescence would produce Röntgen rays. He took some uranium salts which had been made to phosphoresce by exposure, not to Röntgen rays but to sunlight, tested them, and found that they gave out rays possessing properties similar to Röntgen rays. Further investigation showed, however, that to get these rays it was not necessary

to make the uranium phosphoresce, that the salts were just as active if they had been kept in the dark. It thus appeared that the property was due to the metal and not to the phosphorescence, and that uranium and its compounds possessed the power of giving out rays which, like Röntgen rays, affect a photographic plate, make certain minerals phosphoresce, and make gases through which they pass conductors of electricity.

Niepée de Saint-Victor had observed some years before this discovery that paper soaked in a solution of uranium nitrate affected a photographic plate, but the observation excited but little interest. The ground had not then been prepared, by the discovery of the Röntgen rays, for its reception, and it withered and was soon forgotten.

Shortly after Becquerel's discovery of uranium, Schmidt found that thorium possessed similar properties. Then Monsieur and Madame Curie, after a most difficult and laborious investigation, discovered two new substances, radium and polonium, possessing this property to an enormously greater extent than either thorium or uranium, and this was followed by the discovery of actinium by Debierne. Now the researches of Rutherford and others have led to the discovery of so many new radioactive substances that any attempt at christening seems to have been abandoned, and they are denoted, like policemen, by the letters of the alphabet.

Mr. Campbell has recently found that potassium, though far inferior in this respect to any of the substances I have named, emits an appreciable amount of radiation, the amount depending only on the quantity of potassium, and being the same whatever the source from which the potassium is obtained or whatever the elements with which it may be in combination.

The radiation emitted by these substances is of three types known as α , β and γ rays. The α rays have been shown by Rutherford to be positively electrified atoms of helium, moving with speeds which reach up to about one tenth of the velocity of light. The β rays are negatively electrified corpuscles, moving in some cases with very nearly the velocity of light itself, while the γ rays are unelectrified, and are analogous to the Röntgen rays.

The radioactivity of uranium was shown by Crookes to arise from something mixed with the uranium, and which differed sufficiently in properties from the uranium itself to enable it to be separated by chemical analysis. He took some uranium, and by chemical treatment separated it into two portions, one of which was radioactive and the other not.

Next Becquerel found that if these two portions were kept for several months, the part which was not radioactive to begin with regained radioactivity, while the part which was radioactive to begin with had lost its radioactivity. These effects and many others receive a complete explanation by the theory of radioactive change which we owe to Rutherford and Soddy.

According to this theory, the radioactive elements are not permanent, but are gradually breaking up into elements of lower atomic weight; uranium, for example, is slowly breaking up, one of the products being radium, while radium breaks up into a radioactive gas called radium emanation, the emanation into another radioactive substance, and so on, and that the radiations are a kind of swan's song emitted by the atoms when they pass from one form to another; that, for example, it is when a radium atom breaks up and an atom of the emanation appears that the rays which constitute the radioactivity are produced.

Thus, on this view the atoms of the radio-

active elements are not immortal, they perish after a life whose average value ranges from thousands of millions of years in the case of uranium to a second or so in the case of the gaseous emanation from actinium.

When the atoms pass from one state to another they give out large stores of energy, thus their descendants do not inherit the whole of their wealth of stored-up energy, the estate becomes less and less wealthy with each generation; we find, in fact, that the politician, when he imposes death duties, is but imitating a process which has been going on for ages in the case of these radioactive substances.

Many points of interest arise when we consider the rate at which the atoms of radioactive substances disappear. Rutherford has shown that whatever be the age of these atoms, the percentage of atoms which disappear in one second is always the same; another way of putting it is that the expectation of life of an atom is independent of its age—that an atom of radium one thousand years old is just as likely to live for another thousand years as one just sprung into existence.

Now this would be the case if the death of the atom were due to something from outside which struck old and young indiscriminately; in a battle, for example, the chance of being shot is the same for old and young; so that we are inclined at first to look to something coming from outside as the cause why an atom of radium, for example, suddenly changes into an atom of the emanation. But here we are met with the difficulty that no changes in the external conditions that we have as yet been able to produce have had any effect on the life of the atom; as far as we know at present the life of a radium atom is the same at the temperature of a furnace as at that of liquid air—it is not altered by surrounding the radium

by thick screens of lead or other dense materials to ward off radiation from outside, and what to my mind is especially significant, it is the same when the radium is in the most concentrated form, when its atoms are exposed to the vigorous bombardment from the rays given off by the neighboring atoms, as when it is in the most dilute solution, when the rays are absorbed by the water which separates one atom from another. This last result seems to me to make it somewhat improbable that we shall be able to split up the atoms of the non-radioactive elements by exposing them to the radiation from radium; if this radiation is unable to affect the unstable radioactive atoms, it is somewhat unlikely that it will be able to affect the much more stable non-radioactive elements.

The evidence we have at present is against a disturbance coming from outside breaking up of the radioactive atoms, and we must therefore look to some process of decay in the atom itself; but if this is the case, how are we to reconcile it with the fact that the expectation of life of an atom does not diminish as the atom gets older? We can do this if we suppose that the atoms when they are first produced have not all the same strength of constitution, that some are more robust than others, perhaps because they contain more intrinsic energy to begin with, and will therefore have a longer life. Now if when the atoms are first produced there are some which will live for one year, some for ten, some for a thousand, and so on; and if lives of all durations, from nothing to infinity, are present in such proportion that the number of atoms which will live longer than a certain number of years decrease in a constant proportion for each additional year of life, we can easily prove that the expectation of life of an atom will be the same whatever its age may be. On this view the

different atoms of a radioactive substance are not, in all respects, identical.

The energy developed by radioactive substances is exceedingly large, one gram of radium developing nearly as much energy as would be produced by burning a ton of coal. This energy is mainly in the α particles, the positively charged helium atoms which are emitted when the change in the atom takes place; if this energy were produced by electrical forces it would indicate that the helium atom had moved through a potential difference of about two million volts on its way out of the atom of radium. The source of this energy is a problem of the deepest interest; if it arises from the repulsion of similarly electrified systems exerting forces varying inversely as the square of the distance, then to get the requisite amount of energy the systems, if their charges were comparable with the charge on the α particle, could not when they start be further apart than the radius of a corpuscle, 10^{-13} cm. If we suppose that the particles do not acquire this energy at the explosion, but that before they are shot out of the radium atom they move in circles inside this atom with the speed with which they emerge, the forces required to prevent particles moving with this velocity from flying off at a tangent are so great that finite charges of electricity could only produce them at distances comparable with the radius of a corpuscle.

One method by which the requisite amount of energy could be obtained is suggested by the view to which I have already alluded—that in the atom we have electrified systems of very different types, one small, the other large; the radius of one type is comparable with 10^{-13} cm., that of the other is about 100,000 times greater. The electrostatic potential energy in the smaller bodies is enormously greater than that in the larger ones; if one of these

small bodies were to explode and expand to the size of the larger ones, we should have a liberation of energy large enough to endow an α particle with the energy it possesses. Is it possible that the positive units of electricity were, to begin with, quite as small as the negative, but while in the course of ages most of these have passed from the smaller stage to the larger, there are some small ones still lingering in radioactive substances, and it is the explosion of these which liberates the energy set free during radioactive transformation?

The properties of radium have consequences of enormous importance to the geologist as well as to the physicist or chemist. In fact, the discovery of these properties has entirely altered the aspect of one of the most interesting geological problems, that of the age of the earth. Before the discovery of radium it was supposed that the supplies of heat furnished by chemical changes going on in the earth were quite insignificant, and that there was nothing to replace the heat which flows from the hot interior of the earth to the colder crust. Now when the earth first solidified it only possessed a certain amount of capital in the form of heat, and if it is continually spending this capital and not gaining any fresh heat it is evident that the process can not have been going on for more than a certain number of years, otherwise the earth would be colder than it is. Lord Kelvin in this way estimated the age of the earth to be less than 100 million years. Though the quantity of radium in the earth is an exceedingly small fraction of the mass of the earth, only amounting, according to the determinations of Professors Strutt and Joly, to about five grams in a cube whose side is 100 miles, yet the amount of heat given out by this small quantity of radium is so great that it is more than enough to replace the heat which flows from the inside to the

outside of the earth. This, as Rutherford has pointed out, entirely vitiates the previous method of determining the age of the earth. The fact is that the radium gives out so much heat that we do not quite know what to do with it, for if there was as much radium throughout the interior of the earth as there is in its crust, the temperature of the earth would increase much more rapidly than it does as we descend below the earth's surface. Professor Strutt has shown that if radium behaves in the interior of the earth as it does at the surface, rocks similar to those in the earth's crust can not extend to a depth of more than forty-five miles below the surface.

It is remarkable that Professor Milne from the study of earthquake phenomena had previously come to the conclusion that rocks similar to those at the earth's surface only descend a short distance below the surface; he estimates this distance at about thirty miles, and concludes that at a depth greater than this the earth is fairly homogeneous. Though the discovery of radioactivity has taken away one method of calculating the age of the earth it has supplied another.

The gas helium is given out by radioactive bodies, and since, except in beryls, it is not found in minerals which do not contain radioactive elements, it is probable that all the helium in these minerals has come from these elements. In the case of a mineral containing uranium, the parent of radium in radioactive equilibrium, with radium and its products, helium will be produced at a definite rate. Helium, however, unlike the radioactive elements, is permanent and accumulates in the mineral; hence if we measure the amount of helium in a sample of rock and the amount produced by the sample in one year we can find the length of time the helium has been accumulating, and hence the age of the

rock. This method, which is due to Professor Strutt, may lead to determinations not merely of the average age of the crust of the earth, but of the ages of particular rocks and the date at which the various strata were deposited; he has, for example, shown in this way that a specimen of the mineral thorianite must be more than 240 million years old.

The physiological and medical properties of the rays emitted by radium is a field of research in which enough has already been done to justify the hope that it may lead to considerable alleviation of human suffering. It seems quite definitely established that for some diseases, notably rodent ulcer, treatment with these rays has produced remarkable cures; it is imperative, lest we should be passing over a means of saving life and health, that the subject should be investigated in a much more systematic and extensive manner than there has yet been either time or material for. Radium is, however, so costly that few hospitals could afford to undertake pioneering work of this kind; fortunately, however, through the generosity of Sir Ernest Cassel and Lord Iveagh a Radium Institute, under the patronage of his Majesty the King, has been founded in London for the study of the medical properties of radium, and for the treatment of patients suffering from diseases for which radium is beneficial.

The new discoveries made in physics in the last few years, and the ideas and potentialities suggested by them, have had an effect upon the workers in that subject akin to that produced in literature by the Renaissance. Enthusiasm has been quickened, and there is a hopeful, youthful, perhaps exuberant, spirit abroad which leads men to make with confidence experiments which would have been thought fantastic twenty years ago. It has quite dispelled

the pessimistic feeling, not uncommon at that time, that all the interesting things had been discovered, and all that was left was to alter a decimal or two in some physical constant. There never was any justification for this feeling, there never were any signs of an approach to finality in science. The sum of knowledge is at present, at any rate, a diverging not a converging series. As we conquer peak after peak we see in front of us regions full of interest and beauty, but we do not see our goal, we do not see the horizon; in the distance tower still higher peaks, which will yield to those who ascend them still wider prospects, and deepen the feeling, whose truth is emphasized by every advance in science, that "Great are the Works of the Lord."

J. J. THOMSON

ROBERT EDWARDS CARTER STEARNS

DR. ROBERT EDWARDS CARTER STEARNS died at Los Angeles, Cal., July 27, in his eighty-third year. He was a native of Boston, Mass., a son of Charles Stearns, and was born February 1, 1827. He was educated in the public schools, followed by a course of mercantile training, and from his earliest years evinced a deep love of nature, fostered by his father, with whom similar tastes led to a degree of comradeship in rambles and hunting expeditions which he always remembered with appreciation. The boy had an unusual artistic ability, and, though his early avocations were services in a bank and on a farm, when only twenty-two years of age he painted a panorama of the Hudson River from the mouth of the Mohawk to Fort William, which he exhibited with much success. He turned his attention to mining, explored the coal fields of southern Indiana, and in 1854 was appointed resident agent of several copper mines in northern Michigan, on Lake Superior. In 1858 he went to California, where he became a partner in the large printing establishment of a brother-in-law of his wife, in San Francisco. This firm published the

Pacific Methodist, a weekly religious paper, and in the troubled times preceding the civil war the reverend editor of this journal was obliged to visit the east. Stearns was requested to fill this place during his absence. The fate of California hung in the balance, many of the immigrants from the southern states urged independence for that territory when hostilities broke out. Stearns took the responsibility of making his paper an enthusiastic advocate of the union cause, and to this call and the eloquence of Thomas Starr King, old Californians believed the decision of the people to stand by the Union in that struggle was due in no small degree. Through the influence of Justice Field, Stearns was appointed deputy clerk of the supreme court of California in 1862, a post which he resigned in the following year to accept the secretaryship of the State Board of Harbor Commissioners, which he was obliged to resign some years later on account of ill health. Coming to the east, he made one of a party, comprising besides himself the late Dr. William Stimpson and Col. Ezekiel Jewett, for the exploration of the invertebrate fauna of southwestern Florida, during which large collections were made for the Smithsonian Institution. He returned to California, and in 1874 was elected secretary of the University of California, being the business executive of that institution under the presidency of the late Dr. Daniel C. Gilman. He served in this capacity for eight years with great approval, and, when ill health again obliged him to retire from service, the university as expressive of their sense of his services to the cause of education in California, and in recognition of his scientific attainments, conferred upon him the degree of doctor of philosophy. Returning to the east after the death of Mrs. Stearns, he was engaged in researches for the U. S. Fish Commission in 1882, was appointed paleontologist to the U. S. Geological Survey by Major Powell in 1884, and assistant curator of mollusks in the National Museum by Professor Baird. His collection of mollusca was acquired by the museum. Age and infirmity obliged him to return to the more genial climate

of California in 1892, where he settled in Los Angeles, continuing, as his strength permitted, his researches into the malacology of the Pacific coast. He married March 28, 1850, Mary Anne Libby, daughter of Oliver Libby, of Boston, and is survived by a daughter.

Dr. Stearns was an earnest student of mollusks from boyhood; his early experience led him to interest himself in horticulture and landscape gardening, and his ability in this line is attested by the beauty of the university grounds at Berkeley, which were developed under his superintendence. His knowledge of the Pacific coast mollusca was profound, and a long list of papers on this topic and on the shells of Florida was the result. He also contributed many papers on various branches of horticulture and gardening to the California periodicals devoted to this subject. He was an enthusiastic supporter of the California Academy of Sciences in its early days, and, after the earthquake of 1868, when disaster threatened the society, he, with Professor J. D. Whitney and a few other friends, stood between it and dissolution. He was a member of numerous scientific societies at home and abroad, and of the Sons of the Revolution.

Dr. Stearns was a man of sanguine temperament, with a lively sense of humor and high moral character. His reading was wide, his learning never obtrusive, his interest in art, literature and all good causes, intense. He was a staunch friend and, for a righteous object, ever ready to sacrifice his own material interests. His services to Californian science will keep his memory green.

WM. H. DALL

SCIENTIFIC NOTES AND NEWS

At a meeting of the Paris Academy of Sciences on August 18, the permanent secretary gave a eulogy of Professor Simon Newcomb, who was a foreign associate of the academy.

In the last issue of SCIENCE it was noted that at the celebration of the five-hundredth anniversary of the University of Leipzig the degree of doctor of medicine was given to Professor E. B. Wilson, of Columbia University, and the degree of doctor of philosophy to Pro-

fessor Jacques Loeb, of the University of California. The degree of doctor of philosophy was also given to Professor A. A. Michelson, of the University of Chicago. There were in all five degrees given to Americans, more than to the citizens of any foreign country except Austria. The two other degrees were to Mr. Roosevelt and Professor J. W. Burgess, of Columbia University.

CLARK UNIVERSITY will hold its second decennial celebration from September 6 to 11, the exercises being under the auspices of the department of psychology and the department of pedagogy and school hygiene. Those from abroad who will give lectures are Professors L. William Sterns, of Berlin; Dr. C. G. Jung, of Zurich; Dr. Leo Bergerstein, of Vienna, and Dr. Sigmund Freud, of Vienna. Among those from America who give lectures are Dr. Ellsworth Brown, Professor E. B. Titchener, Professor Franz Boas, Professor H. S. Jennings and Professor Adolf Meyer.

THE Paris Academy of Sciences has decided to signalize its appreciation of work in aeronautics by devoting \$4,000 to striking gold and enamel medals which will be presented to foreign and French aeronauts. Medals will be awarded to the Wright brothers, and to MM. Bleriot and Voisin.

IT is stated in *Nature* that the Cracow Academy of Sciences has awarded the Nicolas Copernic prize, amounting to 1,000 crowns, to M. Jean Krassowski, of Cracow, for his treatment of the question, "A l'aide de la méthode de M. A. Schuster, examiner la question si les périodes des variations des latitudes, indiquées par MM. Chandler, Kimura, etc., sont réelles ou non." The Constantin Simon prize, of 900 crowns, for a work in the Polish language on mathematics or physics, has been adjudicated to M. Stanislas Zaremba, for his book "Exposé des premiers Principes de la Théorie des Nombres entiers."

THE Paris correspondent of the *Journal of the American Medical Association* reports that the Academy of Sciences has awarded three prizes of \$500 (2,500 francs) each, one to Dr. Neumann for his researches on the family of the ixodidae and on the various groups of para-

sites of the superior vertebrates; one to Dr. Charles Nicolle for his works on infantile kala-azar, and one to Professor J. Bergonié and Dr. L. Tribondeau, of Bordeaux, for their work on Röntgen rays and fulguration. In the same competition three second prizes of \$300 (1,500 francs) each were awarded, one to Professor H. Truc and Dr. P. Chavernac, of Montpellier for their work on ocular hygiene and ophthalmologic inspection of schools; one to MM. C. Porcher and C. Hervieux for their "Investigations on Indol and Some of its Derivatives"; and one to M. Moussu, professor at the National Veterinary School of Alfort, for his "Investigations on the Tuberculosis of Cattle." The Godard prize (\$200) was awarded to Dr. Pousson, professor at the College of Medicine of Bordeaux, for his "Surgery of Nephritis." The Bellion prize (\$280) was awarded to Dr. C. Nicolas, resident physician of Lifu, New Caledonia, for his work, "Public and Private Hygiene of the Kanakas of the Loyalty Islands." The Bréant prize (\$20,000), offered for a cure for Asiatic cholera, was not awarded; but out of the accumulated interest of this fund the academy awarded a prize of \$800 to W. M. Haffkine for his work on vaccination and bubonic plague, and a second prize of \$200 to Dr. Louis Rénon, physician of the hospital of Paris, for his work entitled "Practical Treatment of Pulmonary Tuberculosis."

PROFESSOR ARMIN O. LEUSCHNER, director of the Students' Observatory of the University of California, has returned from abroad where he spent his sabbatical leave of absence.

M. DARBOUX, the eminent mathematician, permanent secretary of the Paris Academy of Sciences, has been appointed the official delegate to represent France at the approaching Hudson-Fulton celebration.

AT the recent celebration at Leipzig, President J. G. Schurman, of Cornell University, made the address on behalf of American universities.

UNIVERSITY AND EDUCATIONAL NEWS

THE council of the city of Cincinnati has appropriated the sum of \$576,000 to erect three new buildings for the University of Cincinnati.

By the will of Cornelius C. Cuyler, the New York banker and a trustee of Princeton University, \$100,000 is bequeathed to Princeton University. The residue of the estate, which is said to be very large, will go to the university after the death of Mrs. Cuyler.

MR. THOMAS SHEVLIN has given \$60,000 to the University of Minnesota. Of this sum \$10,000 will be used to enlarge Alice Shevlin Hall and \$50,000 to endow five scholarships.

GROUND has been broken for the new wing to the engineering building at the University of Wisconsin. It is to raise the present half-height third story of the west end of the building to a full story, with a half-height fourth story above for drafting rooms. Ultimately a similar wing will be built at the east end of the building. The ground dimensions of the wing are 40 X 70 feet, and the material is the same pressed brick as the main building, with Bedford stone trimming and slate roof.

THE new medical college of the University of Cincinnati—the Ohio-Miami Medical College—will open its first session in the old university building on the McMicken Homestead, which has been completely equipped for the purpose. Dr. P. G. Woolley, of the University of Nebraska, has been appointed professor of pathology, and Dr. William B. Wherry, of the Public Health and Marine-Hospital Service, assistant professor of bacteriology.

AT the University of Chicago associate professors have been appointed from among assistant professors as follows: Carl Kinsley, physics; Charles B. Child, zoology; Anton J. Carlson, physiology, and H. Gideon Wells, pathology.

DR. E. B. BRYAN, president of Franklin College, in Indiana, has been elected president of Colgate University, at Hamilton, N. Y.

DR. R. K. MCCLUNG, for the past two years in charge of the physics department of Mount Allison University, Sackville, N. B., and previously on the physics staff of McGill University, has accepted an appointment to the lectureship of physics in the University of Manitoba, Winnipeg.

SCIENTIFIC BOOKS

Artificial and Natural Flight. By Sir HIRAM MAXIM. New York, The Macmillan Co. \$1.75 net.

Sir Hiram Maxim, the celebrated inventor and manufacturer of the machine gun which bears his name, has published in a book of 165 pages an account of the experiments which he made for and with a flying machine from 1889 to 1894, and he has supplemented this with his own observations and reflections, so as to form a valuable guide to others who may contemplate like work.

The preface and the introductory chapter bestow some hard slaps upon mathematicians who have written upon aviation; for Mr. Maxim believes that practical experiment alone is to be relied upon, and he states that all the successful flying machines of to-day are built upon the lines which he had thought out and found to be the best.

In the chapters upon Air Currents and upon Kites he describes some very interesting observations upon the rising trends of winds with which soaring birds probably perform their astonishing feats. He expresses the belief, however, that "we shall never be able to imitate the flight of the soaring birds. We can not hope to make a sensitive apparatus which will work quick enough to take advantage of the rising currents of air." It is possible that Mr. Maxim is mistaken.

Then he gives an account of his experiments upon screws, on the coefficient of air resistance of various forms, on the best shapes for aeroplanes and for condensers and then passes to hints as to the building of flying machines, which it is greatly to be hoped he will apply himself, now that the success of others has removed the odium which attached to such experiments less than fifteen years ago.

Mr. Maxim discusses briefly the Santos-Dumont, the Farman, the De la Grange and the Blériot flying machines, while he expresses doubts in the main body of the book as to the truth of the "alleged flights" of the Wright Brothers, which he takes back in the appendix. His calculations of resistances and horse power required are somewhat vague; he figures for

the De la Grange machine that at an angle of incidence of 1 in 10 the screw thrust would be 100 pounds, requiring 10.66 H.P. to overcome the drift, and he allows another 10 H.P. for the atmospheric resistance due to the motor, the man and the framework of the machine, reaching the conclusion that of the 50 H.P. developed, 29 H.P. will be consumed in slip. This is probably erroneous. If the cross sectional area of the air-resisting parts be measured and proper coefficient applied it will be found that they require a good deal more than 10 H.P. to overcome the resistance at 40 miles an hour, and that the slip of the screw is much less than estimated.

Mr. Maxim has no good opinion of balloons. He devotes a chapter to them, but expresses the opinion that the day of the balloon is past, evidently not recognizing the fact that the flying machine, while capable of great speed, is limited in size and carrying capacity by its own increase of weight as the cube of its linear dimensions, eventually reaching a size beyond which it is no longer practicable, while the balloon increases in surplus lifting power faster than its own weight and promises some usefulness in spite of its inferior speed, its bulk and its fragility.

Mr. Maxim gives an account, all too brief, of his own flying machine, weighing some 8,000 pounds, with 4,000 to 6,000 square feet of sustaining surfaces, a motor of 363 H.P., this being a steam engine of his own design of unprecedented lightness, developing a thrust at the screws of 2,164 pounds. This wonderful and immense apparatus, the work of a very able engineer, was run very many times over a railway track of nine feet gauge, being restrained from rising more than two feet by guard rails of timber of thirty feet gauge. With this arrangement many tests were made and data obtained preliminary to an attempt at free flight, but on July 31, 1894, the apparatus rose with such force as to burst through the guard rails and enter upon a cruise. Steam was shut off at once and the machine, after flying perhaps 200 feet, fell and was broken. It was repaired, but various circumstances have prevented its being tested again.

There were two causes for this failure. First, the stability was deficient, as subsequently recognized, and second, Mr. Maxim did not and could not know how to handle it in the air, for lack of preliminary practise in free flight. The gradual training which all successful aviators have had to go through during the last five years is an abundant demonstration, and one of the marvels of the evolution is that so few fatal accidents have occurred to the experimenters, although there have been almost innumerable breakages of the machines.

O. CHANUTE

Our Insect Friends and Enemies. By JOHN B. SMITH, ScD., Professor of Entomology in Rutgers College and Entomologist of the New Jersey Agricultural Experiment Station. Philadelphia, J. B. Lippincott Company. 1909. Pp. 314, 1 plate. \$1.50.

The many good insect books which have appeared during the past few years have not entirely filled the need for more literature of the right sort. A careful reading of this book shows it to be quite different in scope from any of its predecessors. The object is not to present a scheme of classification, a manual of insect anatomy, or a handbook of injurious species of insects, yet these phases are treated incidentally and satisfactorily.

In the foreword the author explains that his object has been to present an account of the relation of insects to other living things. In this he has been eminently successful, and it would be difficult indeed in a book of its size to give a more comprehensive and complete general survey of the whole subject. To the student and working entomologist this book is useful, but especially to the lay reader who sometimes gets an exaggerated idea of the value of parasites or remedial measures in destroying noxious species, is this volume of great value because it shows these relations in their true light and perspective.

The average individual has little knowledge about the recent discoveries relating to the transmission of human and other animal diseases by insects, especially flies, mosquitoes and fleas. In fact, his only source of information has been the newspapers, which print oc-

casional disjointed statements regarding this very important matter. Chapter IX. makes this subject plain to anyone who will read it—and everybody ought to read it.

Chapters IV. and V., on the relation of insects to each other and their relation to animals, are particularly good and deserve to have a wide reading.

A critical person might question the statement regarding the formation of galls on page 78: "and the remarkable point is, that the gall is purely a production of the plant, and the insect has apparently nothing at all to do with it." That the irritation, stimulus or injury of the insect which causes a gall of definite and characteristic shape to form on a particular plant is little known, is true, yet it can not be considered that the insect has "apparently nothing at all to do with it," when the attack of each different species of Cynipidae or Cecidomyidae causes a different but entirely characteristic gall to form on the same host plant. However, this is partly explained below on the same page by Professor Smith.

A good colored plate of household insects forms the frontispiece to the volume, which is well printed on good paper, and attractively bound in tan-colored linen.

Of the 121 figures in the text about 35 are new, and were made from excellent pen drawings.

The book is remarkably free from typographical or other errors, the only one noticed being the mis-spelled specific name of the Angoumois grain moth *Gelechia cerealella* Oliv., on page 242.

This volume should find a place in every library of entomological works, and every public library should have a copy.

W. E. BRITTON

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NOTES ON ENTOMOLOGY

MR. G. H. VERRALL has completed another volume in the series of books on the Diptera of Great Britain.¹ This volume includes all

¹ "British Flies," Vol. V., London, 1909, 780 pp., 406 figs.

of the Brachycera except the Empidæ, Dolichopodidæ, Lonchopteridæ and Phoridæ. He gives the characters of all the subfamilies and important European genera, whether occurring in England or not. His arrangement of the brachycerous Diptera is based on that of Osten Sacken, but with several important modifications. It is as follows: Eremochæta (Stratiomyidæ, Leptidæ, Tabanidæ, Nemestrinidæ and Cyrtidæ); Tromoptera (Bombylidæ and Therevidæ); Dermatina (Scenopinidæ and Mydæidæ) Ernegopoda (Apoceridæ and Asilidæ); Microphona (Empidæ and Dolichopodidæ); Acroptera (Lonchopteridæ), and Hypocera (Phoridæ). He puts *Xylomyia* in the Stratiomyidæ and *Xylophagus* in the Leptidæ. In the Asilinæ he uses the various genera proposed, but, recognizing the difficulty of their identification, he also gives a table of the British species based on superficial characters. An important feature is an illustrated article on the larvæ of the Brachycera, and of the Syrphidæ, Platypezidæ and Pipunculidæ by David Sharp, partly translated from Brauer. In the back is a list of the species common to Europe and North America, and as a supplement a catalogue of the Palæarctic Diptera Brachycera (except Dolichopodidæ and Empidæ).

DR. L. G. NEUMANN has issued a useful little book on the parasites and diseases of domestic birds.² It contains much interesting matter on the insects and mites that attack poultry and pigeons, arranged according to the parts affected. There is a short description of each parasite, the nature and appearance of the injury it causes, and the remedies that may be used to combat it. Most of the figures of these forms are original and accurate.

DRS. J. W. W. STEPHENS and S. R. Christophers have prepared a book³ that will be of much help and interest to the many who are now concerned in the study of medical entomology. The entomological part is in two

² "Parasites et Maladies parasitaires des Oiseaux domestiques," Paris, 1909, 230 pp., 89 figs.

³ "The Practical Study of Malaria, and Other Blood Parasites," London, 1908, 414 pp., 128 figs.

chapters on ticks, one on tsetse flies, etc., and fifteen chapters on mosquitoes. In the latter are directions for collecting, rearing, dissection, etc. There are chapters on How to Prepare a Blood Film, How to Detect the Malaria Parasite, How to Make a Malaria Survey, on the Life-history of the Malaria Parasite, on Piroplasma and on Trypanosoma and the diseases produced by them.

DR. J. KENNEL has begun the issuance of a large work on the European Tortricid moths.⁴ Part I. is published, and contains an account of the structure, habits, life-history and variability of the leaf-rollers, and a table for the determination of the 55 genera. There is a long account of the relationships of the genera, and the phylogeny of the groups, illustrated by a plate of wing venation. The systematic part treats of the 43 species of *Acalla*, while the beautiful plates figure the species of this genus and those of *Philedone*, *Epagoge* and *Cacæcia*. There are to be 24 plates in the entire work, which is partially supported by a grant from the Elizabeth Thompson Science Fund.

MR. E. P. VAN DUZEE has published the results of a recent collecting trip to Florida.⁵ Over 330 species were taken, 28 of which are described as new. He finds that the hemipterous fauna of Florida is made up of four elements: (1) the Carolinian, which spread southward from the Georgia mountains; (2) the West Indian, which has extended over the tip of Florida and up each coast; (3) a few forms from Texas and Mexico; and (4) the endemic species, which are very numerous, and largely local adaptations of the Carolinian migrants.

PROFESSOR C. T. VORHIES has issued a useful paper dealing with the immature stages of some Wisconsin caddice-flies.⁶ The larvæ of

⁴ "Die Palæarktischen Tortriciden, Zoologica," Hft. 54, 1908, 4to, 6 pls., 100 pp.

⁵ "Observations on Some Hemiptera taken in Florida in the Spring of 1908," *Bull. Buffalo Soc. Nat. Hist.*, IX., pp. 149-230, 1909.

⁶ "Studies on the Trichoptera of Wisconsin," *Trans. Wisc. Acad. Science*, XVI., pp. 647-718, 10 pls., 1909.

these aquatic insects are but little known in this country, and Professor Vorhies has made a good start in describing 18 species that he reared to adult. Six species are considered new. The figures show the structural characters of the larvæ and also of some of the adults.

THE larvæ of gnats of the genus *Chironomus* have often been studied by naturalists, but not the least interesting is a posthumous paper by A. T. Mundy.¹ The part on the anatomy of the head seems to be particularly well done. There are detailed accounts of the making of the larval tubes, and a summary of the habits of allied *Chironomus* larvæ.

THE largest volume so far published on the "Collections Zoologiques du Baron Edm. de Selys Longchamps" is Fascicle VIII., a monograph of the Ascalaphidæ² by H. van der Weele. The de Selys collection is particularly rich in this family, possessing many types of Latreille and Rambur. About 200 species are now known, arranged in 50 genera, and the author has had peculiar facilities in studying specimens in many museums and collections. To the two former subfamilies, Schizophthalminæ and Holophthalminæ, he adds a new subfamily, Protascalaphinæ, for *Stilbopteryx* and *Albardia*.

IN this same series René Martin has published two fascicles (XIX., XX.) on the dragon-flies of the group *Æschnines*, completing the account of this family. There are about 185 species in 28 genera, mostly belonging to *Æschna* or *Gynacantha*. Many species are described as new, especially from Mr. Martin's collection. NATHAN BANKS

SPECIAL ARTICLES

AN EXPERIMENT IN MUSICAL ESTHETICS

IN the field of psychology, few subjects offer as many difficulties to the investigator as that of esthetics, and in the realm of esthetics, few

¹ "The Anatomy, Habits and Psychology of *Chironomus pusio* Meigen (the Early Stages)," Leicester, England, 1909, pp. 56, 8 plates.

² "Ascalaphiden," Monographisch Bearbeitet, 326 pp., 254 figs., 2 col. plates, Brussels, 1909.

topics are more obscure than those that relate to the art of music.

Problems in musical esthetics, by their very nature, can not be adequately understood without taking account of both their psychological and their purely musical aspects. Unfortunately for the solution of such problems, however, the psychologist and the musician, in too many instances, not only fail to cooperate in their studies, but even lack an appreciative and sympathetic understanding of each other's methods and conclusions. We are much in need of two-handed men, equipped in both directions—or else, of intimate collaboration between the investigators in the separate fields.

Largely because of a lack of just such collaboration, the subject of musical dissonance has been invested with an atmosphere of uncertainty and confusion, in the minds of both psychologists, estheticians and musicians. The difficulties inhere, particularly, in ambiguous definitions of the word *dissonance* itself—definitions to which we have become so accustomed that, as a rule, we fail to notice their inadequacy. Among various uncertain and shifting meanings assigned to the term, two fairly defined conceptions present themselves: (1) a dissonance is a combination of simultaneous tones that *sounds harsh*; (2) a dissonance is a combination of simultaneous tones that *requires resolution* (i. e., creates a feeling of unrest, removable only when the given combination of tones is followed by a more or less prescribed other combination). Either of these definitions is feasible and adequate, in itself; the confusion arises from the fact that, even among men usually careful of their terminology, the word *dissonance* is employed, first in the one sense and then in the other.

It seems to the present writer that the term *dissonance* will be both more nearly exact in its meaning, and more useful to the musician, if it be defined, simply, as a combination of simultaneous tones that *sounds harsh* (psychologically, the sensation produced by such a combination). Under the terms of this definition, the only dissonant intervals are those known as *seconds* and *sevenths*; and the only

dissonant chords, those in which such intervals occur. This definition, it may be added, has been in use in the writer's classes for several years, with satisfactory results.

Objection to this position was recently made, on the ground that, in our experience, there is not so clear a discrimination, as is implied, between *seconds* and *sevenths*, on the one hand, and all the remaining intervals, on the other. Specifically, the claim was advanced that the interval of an *augmented fourth*, traditionally known as a dissonance, sounds equally harsh with the *seconds* and the *sevenths*.

In order to test the truth of these opposite opinions, the writer recently decided to conduct an experiment in which the reactions to various intervals might be determined in the case of persons who, though untrained in musical theory, are yet sensitive to musical effects. A collegiate class was selected for the experiment, a class of music-lovers ignorant of the technique of musical structure.

In order that the validity of the tests might not be affected by any preconceived notions on the part of the performer, the playing of the various intervals employed was entrusted to an assistant, who was kept in total ignorance of the nature and purpose of the experiment. The intervals were played on a grand pianoforte of good quality, and the assistant was instructed, as far as possible, to employ the same touch and intensity throughout the tests. Each interval was played from a manuscript copy; and, after a pause of perhaps two seconds, repeated. Before the following interval was performed, ten seconds, approximately, were allowed to elapse, during which the number of the coming test was announced; it was hoped that, in this way, the various combinations would be isolated. The order of presentation had been previously determined, by lot. Every care was exercised to make the experiment scientifically accurate; so that the results, it is believed, are, in every way, trustworthy.

The following explanation was written on the blackboard before the class: "Assume that all musical intervals (an interval is a com-

bination of two simultaneous tones differing, more or less, in pitch) can be classified in two groups, which we shall call Group X and Group Y. Let Group X include intervals whose sound is *smooth*; let Group Y include intervals whose sound is *harsh*. Three intervals will now be played representing Group X; and afterward three intervals representing Group Y." At this point the assistant was called from an adjoining room, and requested to play the following intervals representing Group X: a *major third*, a *major sixth* and a *minor third*. Afterward, he was requested to play the following intervals representing Group Y: a *major seventh*, a *major second* and a *minor second*.

The actual tests were then presented to the class. These consisted of a series of twenty-two intervals, in which each combination from a *minor second* to a *major seventh* was included twice. The students were requested to record, in writing, the name of the group (X or Y) to which, in their judgment, each interval belonged.

Thirty-five students replied, affording seventy tests in the case of each interval. The results are collated in the following table:

	Smooth	Harsh
Minor second	0	70
Major second	5	65
Minor third	67	3
Major third	68	2
Perfect fourth	56	14
Augmented fourth	68	2
Perfect fifth	53	17
Minor sixth	68	2
Major sixth	69	1
Minor seventh	4	66
Major seventh	2	68

The significant features of the replies are:

1. The almost unanimous verdict that *seconds* and *sevenths* are harsh, whereas all other intervals are smooth.

2. The large number of dissenters from the prevailing opinion that *perfect fourths* and *perfect fifths* are smooth.

3. The almost unanimous verdict that the *augmented fourth* (the same, on the pianoforte, as the *diminished fifth*) is smooth—just as smooth as the *thirds* and *sixths*, and much

more smooth than the *perfect fourth* or the *perfect fifth*.

It is believed that these results are of significance, in connection with a number of problems in the field of musical esthetics.

The experiment described above has stimulated similar tests in other institutions. In Wellesley College, under the direction of Professor H. C. Macdougall, experiments were made by Miss Hetty S. Wheeler, in classes yielding 204 replies. Owing to a typographical error, 306 replies were received in the case of the *major sixth*, and only 102 in the case of the *perfect fifth*. The results from Wellesley College, which are very similar to those described above, are contained in the following table:

	Smooth	Harsh
Minor second	0	204
Major second	14	190
Minor third	199	5
Major third	204	0
Perfect fourth	139	65
Augmented fourth	166	38
Perfect fifth	72	30
Minor sixth	202	2
Major sixth	289	17
Minor seventh	14	190
Major seventh	0	204

Professor W. A. White, of Syracuse University, also, made a similar experiment, with somewhat different results. Inasmuch, however, as his tests were made on classes of students more or less advanced in the study of harmony, many of whom recognized the intervals as they were played, the experiment is obviously not comparable with those previously mentioned.

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THE FORTIETH GENERAL MEETING OF THE
AMERICAN CHEMICAL SOCIETY. II

SECTION OF PHARMACEUTICAL CHEMISTRY

A. B. Stevens, chairman

A New Form of Separator: C. E. PARKER.

The "shaking out" method of extraction is difficult or impracticable with solutions which have marked tendency to emulsify. A separator of flat form is described, in which, when in a horizontal position, the immiscible liquids spread

out in broad thin layers in contact with each other. By gently tilting the separator by manual or mechanical means the layers float about without mixing and the extraction of soluble material is readily effected. The operation may be called "floating out" instead of "shaking out." On placing the separator in erect position the lower liquid may be drawn off through a stopcock as usual.

Investigations of Glacial Phosphoric Acid: L. F. KEBLER and B. HERSTEIN.

It has been known for many years that the composition of glacial phosphoric acid is far from uniform, and its use so far as a chemical reagent and for the manufacture of medicinal products is of questionable utility. Furthermore, solutions of glacial phosphoric acid are comparatively unstable, the metaphosphoric acid reverting to the pyro and the pyro gradually to the ortho. The object of this contribution is: (1) to give a method for determining the respective amounts of the various hydrates of phosphorus pentoxide present in ordinary glacial phosphoric acid; (2) to determine the rapidity of reversion to the higher forms of hydration; (3) to show the undesirability of using it either as a reagent or for preparing medicines.

The Purity of Glycerin: H. C. FULLER and L. F. KEBLER.

In this paper is discussed results of investigations of the various brands of glycerin furnished by manufacturers knowing the object and purpose of such samples. The chief objects of the examinations were: (1) To determine whether or not the tests prescribed by the Pharmacopœia were unduly rigid. (2) Whether or not any glycerin was available which when used in making up Haines's solution would not be instrumental in causing a reduction of the copper.

Note on the Determination of Morphin: C. E. PARKER.

The use of a solution of thymol in chloroform (or other volatile solvent) for extracting morphin from solutions, especially those containing glycerol and small amounts of morphin, is described. Opium preparations are first freed from alcohol and then extracted with chloroform, first in acid or neutral solution and again after addition of excess of potassium hydroxid. The solution is then acidified, excess of sodium bicarbonate added and extracted with the thymol solution. The thymol solution is shaken out with one per cent. hydrochloric acid and the latter evaporated. The

morphin in the residue is determined as silicotungstate according to the method of Bertrand.

Notes on Two Important Alkaloidal Reactions:

H. C. FULLER.

This paper discusses the value of the Vitali reaction and the bichromate sulphuric acid reaction in connection with the identification of small amounts of alkaloids in the forensic analysis of medicinal products.

Experiments were conducted with the residues left by extracting the alkaline solutions of various drugs with petroleum, ether and chloroform. It was shown that alkaloids extracted from belladonna, coca, colchicum, nux vomica and yohimbo gave the Vitali reaction with nitric acid and alcoholic potash. The purple color with bichromate and sulphuric acid was given to a greater or less extent by alkaloids extracted from gelsemium, hydrastis, nux vomica, opium, sanguinaria and yohimbe.

Directions are given for the separation and identification of the alkaloids in a mixture of coca, belladonna and nux vomica, and for distinguishing between strychnin and yohimbin when in small quantities. The difference in reactions between strychnin and the alkaloids of hydrastis, gelsemium, sanguinaria and opium is noted, and emphasis laid on the similarity of the reactions of a mixture of nux vomica and gelsemium to a mixture of belladonna and nux vomica.

The Estimation of Molybdenum Trioxid: B. HERSTEIN.

Various methods for the purpose of determining the degree of purity of chemical reagents containing molybdenum have been given, but these varying methods are not only cumbersome, but unsatisfactory, as to final results. This paper contains a method for precipitating molybdenum from a strongly acid solution by means of thioacetic acid, and converting the purified precipitate by means of ignition to molybdic trioxid in a Gooch crucible. Other sulphur-bearing agents were also employed to precipitate the molybdenum with unsatisfactory results.

Tincture of Iodine: AZOR THURSTON.

An outline of methods for determining iodine, ethyl and hydrogen iodides, potassium iodide and alcohol.

Chemical Manipulations and the Variation of Results: W. A. PEARSON.

The results of a chemical analysis are dependent upon three principal factors: (1) method employed, (2) technic of chemist, (3) accidental

errors. The variation of results due to making reports for a moisture-free sample and a sample as received is discussed, and a recommendation made that results, in some way, should indicate the active principles in the drug as received.

The variation of results obtained in the standardization of deci-normal sulphuric acid is mentioned and the comparative accuracy of two methods shown.

The comparative accuracy of short methods and the widely separated views the public has of the chemist's ability, are mentioned.

The Need of Methods of Analysis of Pharmacopæial Articles: B. L. MURRAY.

In many instances the U. S. Pharmacopœia requires a high degree of purity without giving any quantitative methods of determining the purity. It is suggested that the American Chemical Society, through its Section of Pharmaceutical Chemistry, endeavor to find suitable methods of analysis, where they are now wanting in the Pharmacopœia.

Chemical and Physiological Assay of Aconite: A. B. STEVENS.

Numerous experiments prove that aconite contains, in addition to aconitine, a non-active basic constituent which is much less soluble in ether than in chloroform. Consequently the latter should not be used in the assay of aconite or its preparations. The best method for the assay of aconite is believed to be the present pharmacopœial method, or better, the writer's original method in which the alcoholic extract was evaporated with powdered pumice stone.

Aconite root, under various conditions of age and preservation, were assayed by the method above and compared with the physiological method. The same methods were applied to galenical preparations in their normal condition, as well as to those which had been subjected to experiments with a view to partially decomposing the alkaloid. The results prove that the chemical method can be relied upon to determine the quality of aconite and its official preparations. Individuals are not alike sensitive to the action of aconite, hence the physiological method can not be relied upon for standardization.

The following papers were reported by title:
The Assay of Medicated Plasters: F. B. KILMER.
Pharmacopæial Ash Standards: EDWARD KREMERS.
Note on the Curing of Burdock Root: EDWARD KREMERS.

B. E. CURRY,
Press Secretary

(To be continued)